better than the writer. But we are never tired of contemplating a nature so singularly beautiful as that of Faraday. Whether we think of him as pourtrayed by Dr. Bence Jones, Dr. Tyndall, or Dr. Gladstone, or by Prof. Helmholtz, Dumas, or De la Rive, the picture is still the same;—

> "His life was gentle; and the elements
> So mix'd in him, that Nature might stand up
> And say to all the world, 'This was a Man!'"

W. F. BARRETT

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

American Stone Arrowheads

I AM glad to perceive that Dr. Abbott has called attention to the variations in form among North American stone arrowheads, although he cannot admit the correctness of some of the remarks I have incidentally made on this subject in my "Ancient Stone Implements of Great Britain." At the time when that work went to press, I was not in possession of Dr. Abbott's "Stone Age in New Jersey," if, indeed, it had appeared, and in describing the various North American forms, for want of other authorities I principally referred to Schoolcraft, Squier and Davis, and Prof. Daniel Wilson, some of whom Dr. Abbott appears to consider as somewhat antiquated.

It is not to be supposed that my acquaintance with the collections in the United States should in any way approach that of Dr. Abbott, as I have never visited America. My views as to the prevalence of different types were, however, mainly founded, not on printed descriptions, but on actual specimens preserved in collections in this country, my own among the number; and with all deference to Dr. Abbott, who has kindly promised to send me the specimens on which he bases his remarks, I think it will be found that his views, even if completely applicable to New Jersey, will hardly hold good throughout the northern part of the United States and Canada. The principal points on which he disagrees with me are—

1. As to the arrowheads with a notch at the base on either side constituting a prevailing type in North America.
2. As to the leaf-shaped form being very rare.
3. As to the chipping being for the most part but rough, as compared with that exhibited on the arrowheads found in Britain.

1. As to the first point, Dr. Abbott does not appear to appreciate the difference between "a" and "the;" and though it is possible that other forms of arrowheads are found in America in equal numbers with those such as I have mentioned, yet I think he would agree with me, if he were comparing a series, from North America with one from some other country, in recognising those with a notch at the base on either side as offering one of the prevailing types.

2. As to the leaf-shaped form being very rare, I may observe that among the thirty-eight figures of arrowheads given by Dr. Abbott in his "Stone Age in New Jersey" only one is leaf-shaped, though he mentions the plain leaf-shaped form as the prevailing variety of the leaf-shaped arrowhead and its modifications. In my own collection, comprising upwards of 160 North American arrowheads, there are only six that can be termed leaf-shaped, and but two of these are really typical specimens of the form. These two have come into my possession since my book was written. If not "very rare" I think the form must be regarded as comparatively rare.

3. Lastly, as regards workmanship, Dr. Abbott seems not to have observed that my statement as to the roughness of chipping was qualified with the words "for the most part." Anyone comparing a collection of stone arrowheads from North America with one formed in Britain or Ireland would, I think, at once admit that, as a whole, the latter were more finely and delicately chipped. Or, again, comparing Dr. Abbott's illustrations with mine the same general conclusion will be attained. Even the most elegantly formed American specimens that I have seen cannot compete in delicacy of workmanship with some of the English examples, such, for instance, as those represented in my Figures 317, 318, 319. I can only say that if Dr. Abbott or any other American collector will kindly send me some specimens equally well-chipped, I shall be proud to add them to my collection, and not too proud to acknowledge any errors into which I may have been betrayed through insufficiency of knowledge.

JOHN EVANS

Nash Mills, Hemel Hempsted, Sept. 14

Botanical Terminology

BOTANY is now being taught to large numbers in public and middle-class schools; its admission as a subject of general study can only be justified by its high educational value, not so much as an end, but as a means. Many of the boys who work at it will have no time to continue the study in detail, and the little they know of it will be what they learn at school.

Upon all these grounds it is desirable that there should be no unnecessary or conventional impediments placed in the way or as much education as possible being given in the short time allotted to it. I am desirous of eliciting an opinion as to whether the botanical terminology is or is not an impediment, diminishing the educational value and restricting the scope of the subject. For some years at Rugby I have felt myself hampered with a weariness of names. Is a terminology as polyglot in its derivation as the tower of Babel, and often involving questionable hypotheses of function, or incorrect ideas of morphology, the best instrument for making young pupils observe accurately or reason accurately on observed results? And yet a teacher cannot select the terms he will use, (1) because the books, and especially the English and local floras, imply in their readers a knowledge of a wide terminology; and (2) because, as in the fight against Euclid, so here, the teacher is subject to the examiner, and at present the examination papers of the Science and Art Department, Cambridge Local, &c., are incomprehensible to a boy not bristling with terms.

Allow me specially to call attention to some of what appear to me unnecessary faults in the terms used, and to suggest one or two remedies. The names chosen to denote parts are mainly derived from Latin or Greek, or both, or have no received derivation. Well, even supposing that the words do, by their derivation from the dead language, suggest to those who know it some clue to their meaning, yet how few boys can keep these derivations in their heads, especially in schools in which Latin is not much and Greek not at all taught. Why on earth, when the promoters of science are driving out Greek to let in science, should a knowledge of Greek be a necessary "open sesame" to the correct remembering and spelling of botanical terms?

But the above supposition is seldom correct, the words when translated expressing either a fanciful resemblance, or some pre-adamite stage of botanical knowledge. If the opening through which the pollen-tube passes is already called "little gate," perhaps we must submit to the name, but why call it "micropyle."?

And here perhaps is the place to make a protest against such words as were flooded into the science at the time when the sexuality of plants was first established.

The usefulness of insisting on the generalisation that gamogenesis was common to plants and animals only exists in teaching those who know what gamogenesis in animals means. Does that usefulness exist in the case of boys and girls who learn botany.? In teaching Human Physiology, some subjects are omitted and rightly omitted, from such text-books as Huxley's Elementary Physiology; why then should a knowledge of such subjects be assumed in our botanical terms? The facts of botany can all be taught, as far as my experience of boys and girls goes, without the slightest difficulty; the generalisation will follow of itself, as soon as it is wanted; I only object to its being gratuitously, and almost pruriently, forced on the attention. Who can approve of the use in our elementary floras of the term "hermaphrodite," or the same symbols for pistillate and staminate plants being used which the boys are taught in their astronomy as the recognised symbols for Venus and Mars? To this class belong the tribe of words ending in *—androus* and *—gynous*, which must either be taught as unintelligible gibberish, or explained on gamogenetic analogy. I do not wish to be misunderstood: if you are teaching the physiological subjects in animals, let "honi soit" be your motto, and call a spade a spade; there are worse evils than we dream of arising from obscurantism; but if you are teaching botany, keep to your subject, and do not go out of your way to call a club a spade.

Lastly, many words are utterly confusing: either they seem by their jingling similarity to make the learner and indeed even

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older botanists sure to confuse them, as *loculicidal* and *septicidal* and the troop of words which end in —*tropous*; or they convey a morphologically impossible idea, as *inferior* and *superior* as applied to the ovary. To see how these faults can be avoided, let us inquire why an unusual amount of names are required at all.

(I.) Popular names being vaguely used require to be restricted in their meaning by accurate definition.

(II.) A new name is required for any part to which no name is popularly assigned, either because the thing to be named escapes popular observation, or because two or more things are included in the connotation of the popular term.

(III.) New adjectives, or adjectival-periphrases, are required to express characteristics, or relations of part to part.

Let me briefly suggest some principles, which, while remedying the faults of the old terminology, seem not to clash with these three necessities of the subject.

(*a*) Names for new things to be given in English, *ex. gr.*, the names *calyx* and *corolla* to be taught as *cup* and *crown*: in this we should be only following the German use of Kelch and Krone.

(*b*) Where a part of a thing already named requires a fresh name, the preference to be given to a name framed like the German double words—Kelch-blatt, Staub-blatt—so as to indicate the relation of part to part, thus *cup-leaf*, *leaf-stalk*, *flower-stalk* to be taught instead of *sepal*, *petiole*, and *peduncle*.

(*c*) Short expressions involving English (not Greek) prepositions to be used for adjectives: thus *splitting by mid-ribs*, *on seed-vessel*, *united by dust-pouches*, to be used for *loculicidal*, *epigynous*, *syngenesious*.

(*d*) Where the definitions of the terms is given in numbers, numbers or fractions be used instead of those terms: thus in æstivation, $\frac{2}{5}$ to be used for quincuncial; in cutting of leaves the fraction of the leaf cut to be stated instead of *omnia quæ exeunt*, *in-fid*, *-sect*, and *-partite*.

But it will be said—how will pupils taught thus get on afterwards? The answer is, either they will do no more of the subject than they do at school, in which case they will have got the idea without the obstructions of the terms; or they will care to go on further with the subject, in which case they will learn the terms very quickly, being now familiar with the facts and ideas. In neither case will time have been lost, and the scope of botanical subjects which may be treated in the time will have been doubled.

I must apologise for the length to which this letter has run.

FRANK E. KITCHENER

Rugby, September 16

Hutton's Trigonometrical Tables, for Arcs expressed as portions of the Radius

AT the end of the preface of the first edition of Hutton's Mathematical Tables (1785) is the following postscript:—"P.S.—Since my History of Trigonometrical Tables in the following Introduction was printed, there has been published in the 'Philosophical Transactions' for the year 1784, a paper of mine concerning a project for the trigonometrical tables to be constructed on a new plan, namely, in which the arc of the quadrant is divided into aliquot parts of the radius, or according to the real lengths of the arcs, which construction is now in some degree of forwardness, as myself and several assistants have been closely engaged in the execution of it ever since." And in the succeeding editions, down to the sixth, 1822, there occurs on p. 2 of the Introduction the following remark:—"But the complete reformation would be to express all arcs by their real lengths, namely, in equal parts of the radius decimally divided, according to which method I have nearly completed a table of sines and tangents." Hutton died in 1823, and I can find no further reference to the table in question. I feel pretty certain that it has never been published, and there is no other paper on the same subject (except that in the Phil. Trans., 1784) of Hutton's referred to in Watt's "Bibliotheca" or the Royal Society's Catalogue.

The table was intended to give the sines, tangents, &c., of $\frac{1}{1000000}$, $\frac{2}{1000000}$, &c. (the unit being the arc equal to radius) to seven decimal places, and would be very useful. If it has not been published, perhaps some reader of NATURE might be able to say what has become of the manuscript that was nearly completed.

I may mention that the calculation of such a table was under the consideration of the Tables Committee of the British Association, but it was thought that some other tables were at present more urgently needed.

J. W. L. GLAISHER

Cambridge, Sept. 16

THE "HASSLER" EXPEDITION

WE are indebted to the courtesy of the Editor of the *New York Tribune* for early communication of the following information from Prof. Agassiz's expedition:—

OFF GUATEMALA, July 29, 1872

To Prof. Benjamin Peirce, Superintendent U.S. Coast Survey.

MY DEAR PEIRCE:—Do not be surprised at my few messages. It is about all I can do to take advantage of every opportunity that offers for study and collecting; but I rarely feel sufficiently collected to do any connected writing. I have another new chapter concerning glacial phenomena, gathered during our land journey from Talcahuano to Santiago, but it is so complicated a story that I do not feel equal now to recording the details in a connected statement, while the whole may be put in a few words.

There is a broad valley between the Andes and the coast range, the Valley of Chillan extending from the Gulf of Ancud, or Port Montt, to Santiago, and farther north. This valley is a continuation, upon somewhat higher level, of the channels which, from the Strait of Magellan to Chiloe, separate the islands from the main land, with the sole interruption of Tres Montes, which gives the clue to the whole, as we have here in miniature a valley between the Andes and the coast range. Now this great valley, extending for more than 25 degrees of latitude, is a continuous glacier bottom, showing plainly that for its whole length the great southern ice-sheet has been moving northwards in it. I could find nowhere any indication that glaciers descending from the Andes had crossed this valley and reached the shores of the Pacific. In a few localities only did I notice Andean, *i.e.*, volcanic erratics upon the loose materials filling the old glacier bottom. Between Currillo and Santiago, however, facing the gorge of Tenon, I saw two distinct lateral moraines, parallel to one another, chiefly composed of volcanic boulders, resting upon the old drift, and indicating by their position the course of a large glacier that once poured down from the Andes of Tenon, and crossed the main valley, without, however, extending beyond the eastern slope of the coast range. These moraines are so well marked that they are known throughout the country as the Cerillos of Tenon; but nobody suspects their glacial origin; even the geologists of Santiago assign a volcanic origin to them.

What is difficult to describe in this history are the successive retrograde steps of the great southern ice-field, that, step by step, left to the north of it larger or smaller tracks of the valley free of ice, so that large glacial lakes could be formed, and, in fact, seem always to have existed along the retreating edge of the great southern glacier. The natural consequence is that there are everywhere stratified terraces, without border barriers (as these were formerly the ice that has vanished), resting at successively higher or lower levels, as you move north or south, upon unstratified drift of older date, the northermost end of these terraces being the oldest, while those farther south belong to the latter steps in the waning of the ice-fields. From these data I infer that my suggestion concerning the trend of the striæ upon the polished and glaciated surfaces of the vicinity of Talcahuano, alluded to in the postcript of my last letter, is probably correct.

I was much grieved on reaching Valparaiso to hear of the mishaps of the dredging apparatus. The subsequent departure of Pourtales has been a great loss to us all, for notwithstanding his silent nature, he is a powerful standby.

Our visit to the Galapagos has been full of geological

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